JERZY NEYMAN

April 16, 1894–August 5, 1981

BY E. L. LEHMANN

DURING THE 1930s Jerzy Neyman developed a new paradigm for theoretical statistics, which derives optimal statistical procedures as solutions to clearly stated mathematical problems. He applied these ideas to the theories of hypothesis testing, estimation by confidence intervals, and survey sampling. During the following decades this became the dominant approach to theoretical statistics. In addition to his scientific work, Neyman was a far-seeing and highly efficient administrator who in the decade 1945–55 created in Berkeley a substantial Department of Statistics of international stature. Starting in 1945 he also established a Berkeley series of Symposia on Mathematical Statistics and Probability, meeting at five-year intervals, which for the next twenty-five years became the principal series of international meetings in statistics.

Neyman's long life was dominated by his work, of which he took a comprehensive view encompassing its academic, administrative, and social aspects.

Adapted from a biographical article written for the *Dictionary of Scientific Biography*, Supplement II. I am grateful to the American Council of Learned Societies for permitting the use of this material.

J. Rojo (ed.), *Selected Works of E. L. Lehmann*, Selected Works in Probability and Statistics, DOI 10.1007/978-1-4614-1412-4_71, © Springer Science+Business Media, LLC 2012

EARLY YEARS

Jerzy Neyman was born in Bendery (Russia) to parents of Polish ancestry. His full name with title—Splawa-Neyman—the first part of which he dropped at age thirty, reflects membership in the polish nobility. Neyman's father Czeslaw, who died when Jerzy was twelve, was a lawyer and later judge and an enthusiastic amateur archeologist. Since the family had been prohibited by the Russian authorities from living in Central Poland, then under Russian domination, Neyman grew up in Russia: in Kherson, Melitopol, Simferopol, and (after his father's death) Kharkov, where in 1912 he entered the university.

At Kharkov, Neyman was first interested in physics, but because of his clumsiness in the laboratory he abandoned it in favor of mathematics. On reading Lebesgue's "Leçons sur L'integration et la Recherche des Fonctions Primitives," he later wrote (in a Festschrift in honor of Herman Wold [1970]): "I became emotionally involved. I spent the summer of 1915 at a country estate, coaching the son of the owner. There were three summer houses on the estate, filled with young people, including girls whom I found beautiful and most attractive. However, the involvement with sets, measure and integration proved stronger than the charms of young ladies and most of my time was spent either in my room or on the adjacent balcony either on study or on my first efforts at new results, intended to fill in a few gaps that I found in Lebesgue." A manuscript on Lebesgue integration (500 pp., handwritten) that Neyman submitted to a prize competition won a gold medal.

One of his mentors at Kharkov was Serge Bernstein who lectured on probability theory and statistics (including application of the latter to agriculture), subjects that did not particularly interest Neyman. Nevertheless, he later acknowledged the influence of Bernstein from whom he "tried to acquire his tendency of concentrating on some 'big problem'." It was also Bernstein who introduced him to Karl Pearson's *Grammar of Science,* which made a deep impression. After the first World War, Poland regained its independence but soon became embroiled in a war with Russia over borders. Neyman, still in Kharkov, was jailed as an enemy alien. In 1921, in an exchange of prisoners, he finally went to Poland for the first time at the age of twenty-seven.

In Warsaw he established contact with Sierpinski, one of the founders of the journal *Fundamenta Mathematicae,* which published one of Neyman's gold medal results (1923, vol. 5, pp. 328–30). Although Neyman's heart was in pure mathematics, the statistics he had learned from Bernstein was more marketable and enabled him to obtain a position as (the only) statistician at the Agricultural Institute in Bydgoszcz (formerly Bromberg). There, during 1921–22, he produced several papers on the application of probabilistic ideas to agricultural experimentation. In light of Neyman's later development, this work is of interest because of its introduction of probability models for the phenomena being studied, particularly a randomization model for the case of a completely randomized experiment. (A key section was translated and published with an introduction by Speed and comments by Rubin in *Statistical Science,* vol. 5, 1990, pp. 463–80.) He had learned the philosophy of such an approach from Karl Pearson's book, where great stress is laid on models as mental constructs the formulation of which constitutes the essence of science.[1]

In December 1922 Neyman gave up his job in Bydgoszcz to take charge of equipment and observations at the State Meteorological Institute, a change that enabled him to move to Warsaw. He did not like the work and soon left to become an assistant at the University of Warsaw and Special Lecturer in mathematics and statistics at the Central College of Agriculture; he also gave regular lectures at the University of Krakow. In 1924 he obtained his doctorate from the University of Warsaw with a thesis based on the papers he had written at Bydgoszcz.

Since no one in Poland was able to gauge the importance of his statistical work (he was "sui generis," as he later described himself), the Polish authorities provided an opportunity for him to establish his credibility through publication in British journals. For this purpose they gave him a fellowship to work with Karl Pearson in London. He did publish three papers in *Biometrika* (based in part on his earlier work), but scientifically the academic year (1925–26) spent in Pearson's laboratory was a disappointment. Neyman found the work of the laboratory old-fashioned and Pearson himself surprisingly ignorant of modern mathematics. (The fact that Pearson did not understand the difference between independence and lack of correlation led to a misunderstanding that nearly terminated Neyman's stay at the laboratory.) So when, with the help of Pearson and Sierpinski, Neyman received a Rockefeller fellowship that made it possible for him to stay in the West for another year, he decided to spend it in Paris rather than in London.

There he attended the lectures of Lebesgue and a seminar of Hadamard. "I felt that this was real mathematics worth studying" he wrote later, "and, were it not for Egon Pearson, I would have probably drifted to my earlier passion for sets, measure and integration, and returned to Poland as a faithful member of the Warsaw school and a steady contributor to *Fundamenta Mathematicae*."

THE NEWMAN–PEARSON THEORY

What pulled Neyman back into statistics was a letter he received in the fall of 1926 from Egon Pearson, Karl Pearson's son, with whom Neyman had had only little contact in London. Egon had begun to question the rationale underlying some of the current work in statistics, and the letter outlined his concerns. Correspondence developed and, reinforced by occasional joint holidays, continued even after the end of the Rockefeller year when Neyman returned to a hectic and difficult life in Warsaw. He continued to lecture at the university (as docent after his habilitation in 1928), at the Central College of Agriculture, and at the University of Krakow. In addition, he founded a small statistical laboratory at the Nencki Institute for Experimental Biology. To supplement his meager academic income, and to provide financial support for the students and young co-workers in his laboratory, he took on a variety of consulting jobs. These involved different areas of application, with the majority coming from agriculture and from the Institute for Social Problems, the latter work being concerned with Polish census data.

Neyman felt harassed, and his financial situation was always precarious. The bright spot in this difficult period was his work with the younger Pearson. Trying to find a unifying, logical basis that would lead systematically to the various statistical tests that had been proposed by Student and Fisher was a "big problem" of the kind for which he had hoped since his student days with Bernstein.

In 1933 Karl Pearson retired from his chair at University College, London, and his position was divided between R. A. Fisher and Egon Pearson. The latter lost no time, and, as soon as it became available, in the spring of 1934 offered Neyman a temporary position in his laboratory. Neyman was enthusiastic. This would greatly facilitate their joint work and bring relief to his Warsaw difficulties.

The set of issues addressed in the joint work of Neyman and Pearson between 1926 and 1933 turned out indeed to be a "big problem," and their treatment of it established a new paradigm that changed the statistical landscape. What concerned Pearson when he first approached Neyman in 1926 was the ad hoc nature of the small sample tests being studied by Fisher and Student. In his search for a general principle from which such tests could be derived, he had written to Student. In his reply Student suggested that one would be inclined to reject a hypothesis under which the observed sample is very improbable, "if there is an alternative hypothesis which will explain the occurrence of the sample with a more reasonable probability" (E. S. Pearson in *Research Papers in Statistics,* F. N. David, ed., 1966). This comment led Pearson to propose to Neyman the likelihood ratio criterion, in which the maximum likelihood of the observed sample under the alternatives under consideration is compared to its value under the hypothesis. During the next year Neyman and Pearson studied this and other approaches, and worked out likelihood ratio tests for some important examples. They published their results in 1928 in a fundamental two-part paper, "On the Use and Interpretation of Certain Test Criteria for Purposes of Statistical Inference." The paper contained many of the basic concepts of what was to become the Neyman-Pearson Theory of Hypothesis Testing, such as the two types of error, the idea of power, and the distinction between simple and composite hypotheses.

Although Pearson felt that the likelihood ratio provided the unified approach for which he had been looking, Neyman was not yet satisfied. It seemed to him that the likelihood principle itself was somewhat ad hoc and had no fully logical basis. However, in February 1930 he was able to write Pearson that he had found "a rigorous argument in favour of the likelihood method." His new approach consisted of maximizing the power of the test, subject to the condition that under the hypothesis (assumed to be simple) the rejection probability has a preassigned value (the level of the test). He reassured Pearson that in all cases he had examined so far this logically convincing test coincided with the likelihood ratio test. A month later Neyman announced to Pearson that he now had a general solution of the problem of testing a simple hypothesis against a simple alternative. The result in question is the "Fundamental Lemma," which plays such a crucial role in the Neyman-Pearson theory.

The next step was to realize that in the case of more than one alternative there might exist a uniformly most powerful test that would simultaneously maximize the power for all of them. If such a test exists, Neyman found, it coincides with the likelihood ratio test, but in the contrary case—alas—the likelihood ratio test may be biased. These results, together with many examples and elaborations, were published in 1933 under the title, "On the Problem of the Most Efficient Tests of Statistical Hypotheses." While in the 1928 paper the initiative and insights had been those of Pearson, who had to explain to Neyman what he was doing, the situation was now reversed, with Neyman as leader and Pearson a somewhat reluctant follower.

The 1933 paper is the fundamental paper in the theory of hypothesis testing. It established a framework for this theory[2] and states the problem of finding the best test as a clearly formulated, logically convincing mathematical problem that one can then proceed to solve. Its importance transcends the theory of hypothesis testing since it also provided the inspiration for Wald's later, much more general, statistical decision theory.

SURVEY SAMPLING AND CONFIDENCE ESTIMATION

The following year Neyman published another landmark paper. An elaboration of work on survey sampling he had done earlier for the Warsaw Institute for Social Problems, it was directed toward bringing clarity into a somewhat muddled discussion about the relative merits of two different sampling methods. His treatment, described by Fisher as "luminous," introduced many important concepts and results and may be said to have initiated the modern theory of survey sampling.

The year 1935 brought two noteworthy events. The first was Neyman's appointment to a permanent position as reader (associate professor) in Pearson's department. Although at the time he was still hoping to eventually return to Poland, he in fact never did except for brief visits. The second event was the presentation at a meeting of the Royal Statistical Society of an important paper on agricultural experimentation in which he raised some questions concerning Fisher's Latin square design. This caused a break in their hitherto friendly relationship and was the beginning of lifelong disputes. (Fisher, who opened the discussion of the paper, stated that "he had hoped that Dr. Neyman's paper would be on a subject with which the author was fully acquainted, and on which he could speak with authority, as in the case of his address to the Society last summer. Since seeing the paper, he had come to the conclusion that Dr. Neyman had been somewhat unwise in his choice of topics.")

Neyman was to remain in England for four years (1934–38). During this time he continued his collaboration with

Egon Pearson on the theory and applications of optimal tests, efforts that also included contributions from graduate and postdoctoral students. To facilitate publication and to emphasize the unified point of view underlying this work, Neyman and Pearson set up a series, "Statistical Research Memoirs," published by University College and restricted to work done in the Department of Statistics. A first volume appeared in 1936 and a second in 1938.

Another central problem occupying Neyman during his London years was the theory of estimation: not point estimation in which a parameter is estimated by a unique number, but estimation by means of an interval or more general set in which the unknown parameter can be said to lie with specified confidence (i.e., probability). Such confidence sets are easily obtained under the Bayesian assumption that the parameter is itself random with a known probability distribution, but Neyman's aim was to dispense with such an assumption that he considered arbitrary and unwarranted.

Neyman published brief accounts of his solution to this problem in 1934 and 1935 and the theory in full generality in 1937 in "Outline of a Theory of Statistical Estimation Based on the Classical Theory of Probability." He had first submitted this paper to *Biometrika*, then being edited by Egon Pearson, but to his great disappointment Pearson rejected it as too long and too mathematical.

Neyman's approach was based on the idea of obtaining confidence sets $S(X)$ for a parameter θ from acceptance regions for the hypotheses that $\theta = \theta_0$ by taking for $S(X)$ the set of all parameter values θ_0 that would be accepted at the given level. This formulation established an equivalence between confidence sets and families of tests and enabled him to transfer the test theory of the 1933 paper lock, stock, and barrel to the theory of estimation. (Unbeknownst to Neyman, the idea of obtaining confidence sets by inverting an acceptance rule had already been used in special cases by Laplace, in a large-sample binomial setting, and by Hotelling).

In his paper on survey sampling, Neyman had referred to the relationship of his confidence intervals to Fisher's fiducial limits, which appeared to give the same results, although derived from a somewhat different point of view. In the discussion of the paper, Fisher welcomed Neyman as an ally in the effort to free statistics from unwarranted Bayesian assumptions. He went on to say that "Dr. Neyman claimed to have generalized the argument of fiducial probability, and he had every reason to be proud of the line of argument he had developed for its perfect clarity. The generalization was a wide and very handsome one, but it had been erected at considerable expense. . . ." He then proceeded to indicate the disadvantages he saw in Neyman's formulation, of which perhaps the most important one (revealing the wide difference between their interpretations) was nonuniqueness. The debate between the two men over their respective approaches continued for many years, usually in less friendly terms; it is reviewed by Neyman in "Silver Jubilee of My Dispute with Fisher" (*Journal of the Operation Research Society of Japan*, 1961, pp. 145-54).

STATISTICAL PHILOSOPHY

During the period of his work with Pearson, Neyman's attitude toward probability and hypothesis testing gradually underwent a radical change. In 1926 he tended to favor a Bayesian approach[3] in the belief that any theory would have to involve statements about the probabilities of various alternative hypotheses and hence an assumption of prior probabilities. In the face of Pearson's (and perhaps also Fisher's) strongly anti-Bayesian position, he became less certain, and in his papers of the late 1920s (both alone and jointly with Pearson) he presented Bayesian and non-Bayesian approaches side by side. A decisive influence was von Mises's book *Wahrscheinlichkeit, Statistik und Wahrheit* (1928) about which he later wrote (in his Author's Note to *A Selection of Early Statistical Papers of J. Neyman*) that it "confirmed him as a radical 'frequentist' intent on probability as a mathematical idealization of relative frequency." He remained an avowed frequentist and opposed any subjective approach to science for the rest of his life.

A second basic aspect of Neyman's work from the 1930s on is a point of view, that he formulated clearly in the closing pages of his presentation at the 1937 Geneva Conference ("L'estimation statistique traitée comme un problème classique de probabilité," *Actualites Scientifiques et Industrielles*, no. 739, pp. 25–57). He states that his approach is not based on inductive reasoning but on the concept of "comportement inductif" or inductive behavior. That is, statistics is to be used not to extract from experience "beliefs" but as a guide to appropriate action.

He summarized his views in a paragraph in a paper presented in 1949 to the International Congress on the Philosophy of Science ("Foundations of the General Theory of Estimation," *Actualites Scientifiques et Industreilles*, 1951, No. 1146, p. 85).

"Why abandon the phrase 'inductive reasoning' in favor of 'inductive behavior'?" As explained in 1937,[4] the term inductive reasoning does not seem appropriate to describe the new method of estimation because all the reasoning behind this method is clearly deductive. Starting with whatever is known about the distribution of the observable variables X, we deduce the general form of the functions f(X) and g(X) which have the properties of confidence limits. Once a class of such pairs of functions is found, we formulate some properties of these functions which may be considered desirable and deduce either the existence or non-existence of an "optimum" pair, etc. Once the various possibilities are investigated we may decide to use a particular pair of confidence limits for purposes of statistical estimation. This decision, however, is not 'reasoning'. This is an act of will just as the decision to buy insurance is an act of will. Thus, the mental processes behind the new method of estimation consist of deductive reasoning and of an act of will. In these circumstances the term 'inductive reasoning' is out of place and, if one wants to keep the adjective 'inductive', it seems most appropriate to attach to it the noun 'behavior'.

In other writings (e.g., *Reviews of the International Statistical Institute,* 1957, vol. 25, pp. 7–22), Neyman acknowledges that a very similar point of view was advocated by Gauss and Laplace. It is, of course, also that of Wald's later general statistical decision theory. On the other hand, this view was strongly attacked by Fisher (e.g., JRSS (B), 1955, vol. 17, pp. 69-78) who maintained that decision making has no role in scientific inference and that his fiducial argument provides exactly the mechanism required for this purpose.

MOVING TO AMERICA

By 1937 Neyman's work was becoming known not only in England and Poland but also in other parts of Europe and in America. He gave an invited talk about the theory of estimation at the International Congress of Probability in 1937 in Geneva, and in the spring of 1937 he spent six weeks in the United States on a lecture tour organized by S. S. Wilks. The visit included a week at the Graduate School of the Department of Agriculture in Washington arranged by Edward Deming. There he gave three lectures and six conferences on the relevance of probability theory to statistics and on his work in hypothesis testing, estimation, sampling, and agricultural experimentation as illustrations of this approach. These "Lectures and Conferences on Mathematical Statistics," which provided a coherent statement of the new paradigm he had developed and exhibited its successful application to a number of substantive problems, were a tremendous success. A mimeographed version appeared in 1938 and was soon sold out. Neyman published an augmented second edition in 1952.

After his return from the United States, Neyman debated whether to remain in England, where he had a permanent position but little prospect of promotion and independence, or to return to Poland. Then completely unexpectedly, in the fall of 1937, he received a letter from an unknown, G. C. Evans, chairman of the Mathematics Department at Berkeley, offering him a position in the department. Neyman hesitated for some time. California and its university were completely unknown quantities to him, while the situation in England, although not ideal, was reasonably satisfactory and stable. An attractive aspect of the Berkeley offer was the nonexistence there of any systematic program in statistics, so that he would be free to follow his own ideas. What finally tipped the balance in favor of Berkeley was the threat of war in Europe. Thus, in April 1938 Neyman decided to accept the Berkeley offer and emigrate to America, with his wife Olga (from whom he later separated) and his two-year-old son Michael. He had just turned forty-four, and he would remain in Berkeley for the rest of his life.

THE BERKELEY DEPARTMENT OF STATISTICS

Neyman's top priority after his arrival in Berkeley was the development of a statistics program, that is, a systematic set of courses and a faculty to teach them. He quickly organized a number of core courses and began to train some graduate students and one temporary instructor in his own approach to statistics. Administratively, he set up a statistical laboratory as a semiautonomous unit within the Mathematics Department. However, soon America's entry into World War II in 1941 put all further academic development on hold. Neyman took on war work, and for the next several years this became the laboratory's central and all-consuming activity. The work dealt with quite specific military problems and did not produce results of lasting interest.

The building of a faculty began in earnest after the war, and by 1956 Neyman had established a permanent staff of twelve members, many his own students, but also including three senior appointments from outside (Loève, Scheffé, and Blackwell). Development of a substantial faculty, with the attendant problems of space, clerical staff, summer support, and so on, represented a major sustained administrative effort. A crucial issue in the growth of the program concerned the course offerings in basic statistics by other departments. Although these involved major vested interests, Neyman gradually concentrated the teaching of statistics within his program, at least at the lower division level. This was an important achievement both in establishing the identity of the program and in obtaining the student base that alone could justify the ongoing expansion of the faculty. In his negotiations with the administration, Neyman was strengthened by the growing international reputation of his laboratory and by the increasing postwar importance of the field of statistics itself.

An important factor in the laboratory's reputation was the series of international "Symposia on Mathematical Statistics and Probability" that Neyman organized at five-year intervals during 1945–70, and the subsequent publication of their proceedings. The first symposium was held in

August 1945 to celebrate the end of the war and "the return to theoretical research" after years of war work. The meeting, although rather modest compared to the later symposia, was such a success that Neyman soon began to plan another one for 1950. In later years the symposia grew in size, scope, and importance and did much to establish Berkeley as a major statistical center.

The spectacular growth Neyman achieved for his group required a constant struggle with various administrative authorities, including those of the Mathematics Department. To decrease the number of obstacles, and also to provide greater visibility for the statistics program, Neyman, soon after his arrival in Berkeley, began a long effort to obtain independent status for his group as a Department of Statistics. A separate department finally became a reality in 1955, with Neyman as its chair. He resigned the chairmanship the following year (but retained the directorship of the laboratory to the end of his life). He wrote in his letter of resignation that "the transformation of the old Statistical Laboratory into a Department of Statistics closed a period of development . . . and opened a new phase." In these circumstances he stated that "it is only natural to have a new and younger man take over."

There was perhaps another reason. Much of Neyman's energy during his nearly twenty years in Berkeley had gone into administration. His efforts had been enormously successful—a first-rate department, the symposia, and a large number of grants providing summer support for faculty and students. It was a great accomplishment and his personal creation, but now it was time to get back more fully into research.

APPLIED STATISTICS

Neyman's theoretical research in Berkeley was largely motivated by his consulting work, one of the purposes for which the university had appointed him and through which he made himself useful to the campus at large. Problems in astronomy, for example, led to the interesting insight (1948, with E. L. Scott) that maximum likelihood estimates may cease to be consistent if the number of nuisance parameters tends to infinity with increasing sample size. Also, to simplify maximum likelihood computations, which in applications frequently became very cumbersome, he developed linearized, asymptotically equivalent methods—his BAN (best asymptotically normal) estimates (1949), which have proved enormously useful.

His major research efforts in Berkeley were devoted to several large-scale applied projects. These included questions regarding competition of species (with T. Park), accident proneness (with G. Bates), the distribution of galaxies and the expansion of the universe (with C. D. Shane and particularly Elizabeth Scott, who became a steady collaborator and close companion), the effectiveness of cloud seeding, and a model for carcinogenesis. Of these perhaps the most important was the work in astronomy, where the introduction of the Neyman-Scott clustering model brought new methods into the field that "were remarkable and perhaps have not yet been fully appreciated and exploited." (*Peeble: Large Scale Structure of the Universe,* Princeton Univeristy Press, Princeton, N.J., 1979).

Neyman's applicational work, although it extends over many different areas, exhibits certain common features, which he made explicit in some of his writings and which combine into a philosophy for applied statistics. The following are some of the principal aspects:

1. *The studies are indeterministic.* Neyman pointed out that the distinction between deterministic and indeterministic studies lies not so much in the nature of the phenomena as in the treatment accorded to them ("Indeterminism in Science and New Demands on Statisticians," *Journal of the American Statistical Assocication,* 1960, vol. 55, pp. 625–39). In fact, many subjects that traditionally were treated as deterministic are now being viewed stochastically. Neyman himself contributed to this change in several areas.

2. *An indeterministic study of a scientific phenomenon involves the construction of a stochastic model.* In this connection, Neyman introduced the important distinction between models that are interpolatory devices and those that embody genuine explanatory theories. The latter he describes as "a set of reasonable assumptions regarding the mechanism of the phenomena studied," while the former "by contrast consist of the selection of a relatively ad hoc family of functions, not deduced from underlying assumptions, and indexed by a set of parameters" ("Stochastic Models and Their Application to Social Phenomena," coauthored by W. Kruskal and presented at a joint session of the IMS, ASA, and the American Sociological Society, Sept. 1956). The distinction was discussed earlier, and again later, in Neyman's paper in *Annals of Mathematical Statistics* (1939, vol. 10, pp. 372–73) and in *A View of Biometry.* Siam. (Philadelphia, 1974, pp. 185–201).

Neyman's favorite example of the distinction between the two types was the family of Pearson curves, which for a time was very popular as an interpolatory model that could be fitted to many different data sets, and Mendel's model for heredity. "At the time of its invention," he wrote about the latter in the *Journal of the American Statistical Association* (1960, vol. 55, pp. 625–39), "this was little more than an interpolatory procedure invented to summarize Mendel's experiments with peas," but eventually it turned out to satisfy Neyman's two criteria for a model of genuine scientific value, namely (i) broad applicability (i.e., in Mendel's case not restricted to peas) and (ii) identifiability of details (the genes as entities with identifiable location on the chromosomes).

Most actual modeling, Neyman pointed out, is intermediate between these two extremes, often exhibiting features of both kinds. Related is the realization that investigators will tend to use as building blocks models that "partly through experience and partly through imagination, appear to us familiar and, therefore, simple." (with E. L. Scott, "Stochastic Models of Population Dynamics," *Science* 1959, vol. 130, pp. 303–8).

3. *To develop a "genuine explanatory theory" requires substantial knowledge of the scientific background of the problem.* When the investigation concerns a branch of science with which the statistician is unfamiliar, this may require a considerable amount of work. For his collaboration with Scott in astronomy, Neyman studied the astrophysical literature, joined the American Astronomical Society, and became a member of the Commission on Galaxies of the International Astronomical Union. When he developed an interest in carcinogenesis, he spent three months at the National Institutes of Health to learn more about the biological background of the problem.

Neyman summarized his own attitude toward this kind of research in the closing sentence of his paper, "A Glance of My Personal Experiences in the Process of Research" (in *Scientists at Work,* Almquist and Wicksell, Uppsala, Sweden, 1970): "The elements that are common . . . seem to be my susceptibility to becoming emotionally involved in other individuals' interests and enthusiasm, whether these individuals are sympathetic or not, and, particularly in the more recent decades, the delight I experience in trying to fathom the chance mechanisms of phenomena in the empirical world."

An avenue for learning about the state of the art in a field and bringing together diverse points of view, which Neyman enjoyed and of which he made repeated use, was to arrange a conference. Two of these (on weather modification in 1965 and on molecular biology in 1970) became parts of the then-current symposia. In addition, in 1961, jointly with Scott, he arranged a "Conference on the Instability of Systems of Galaxies." In 1973 he edited for the National Academy of Sciences a volume in celebration of the five-hundredth anniversary of the birth of Nicholas Copernicus: *The Heritage of Copernicus: Theories Pleasing to the Mind.* The volume presents revolutionary changes that occurred, respectively, in astronomy and cosmology, biology, chemistry and physics, mathematics, technology, and in the thinking in mathematics and science brought about by the introduction of chance mechanisms. Finally, in July 1981, jointly with Le Cam and on very short notice, Neyman arranged an interdisciplinary cancer conference.

EPILOGUE

A month after the cancer conference, Neyman died of heart failure at age eighty-seven on August 5, 1981. He had been in reasonable health up to two weeks earlier and on the day before his death was still working in the hospital on a book on weather modification.

Neyman is recognized as one of the founders of the modern theory of statistics, whose work on hypothesis testing, confidence intervals, and survey sampling has revolutionized both theory and practice. His enormous influence on the development of statistics is further greatly enhanced through the large number of his Ph.D. students. These are:

From Poland: Kolodziejczyk, Iwaszkiewicz, Wilenski, Pytkowski.

From London: Hsu, David, Johnson, Eisenhart, Sukhatme, Beall, Sato, Shanawany, Jackson, Tang.

From Berkeley: Chen, Dantzig, Lehmann, Massey, Fix, Chapman, Eudey, Gurland, Hodges, Seiden, Hughes, Taylor, Jeeves, Le Cam, Tate, Chiang, Agarwal, Sane, Read, Singh, Borgman, Marcus, Buehler, Kulkarni, Davies, Clifford, Samuels, Oyelese, Ray, Grieg, Green, Tsiatis, Singh, Javitz, Darden.

Neyman was completely and enthusiastically dedicated to his work, which filled his life—there was no time for hobbies. Work, however, included not only research and teaching but also their social aspects, such as traveling to meetings and organizing conferences. Pleasing his guests was an avocation; his hospitality had an international reputation. In his laboratory he created a family atmosphere that included students, colleagues, and visitors, with himself as paterfamilias.

As administrator, Neyman was indomitable. He would not take "no" for an answer and was quite capable of resorting to unilateral actions. He firmly believed in the righteousness of his causes and found it difficult to understand how a reasonable person could disagree with him. At the same time, he had great charm that often was hard to resist.

High on Neyman's list of values were his opposition to injustice and his sympathy for the underdog. Two illustrations must suffice. In 1946, as a member of the allied mission to supervise the Greek elections, his feeling for fairness and justice led him to disobey orders of his superiors, with the consequence of his services being abruptly terminated. The details are given by Constance Reid (*Neyman—from Life*, Springer, New York, 1982, pp. 201–8). This book also describes (pp. 262–68) how deeply Neyman was affected, when in 1963 as a visiting lecturer for the Mathematical Association of America, he came into firsthand contact with segregation in the South. This led him to various efforts that culminated in the establishment at Berkeley of a special scholarship program to help prepare talented underprivileged young people for a university education.

On a personal level, the characteristic that perhaps remains above all in the minds of his friends and associates is his generosity—furthering the careers of his students, giving credit and doing more than his share in collaboration, and extending his help (including financial assistance out of his own pocket) to anyone who needed it.

NOTES

1. "One of my favorite ideas," Neyman later wrote in 1957 (*Rev. Int. Stat. Inst.* 25:8), "learned from Mach via Karl Pearson's 'Grammar of Science,' is that scientific theories are no more than models of natural phenomena."

2. Its role in today's statistical climate is discussed by Lehmann, "The Neyman-Pearson theory after 50 Years" (In *Proceedings of the Berkeley Conference in Honor of Jerzy Neyman and Jack Kiefer*, vol. 1 [Wadsworth, 1985]:1–14).

3. In "Frequentist Probability and Frequentist Statistics" (*Synthese* 36[1977]:97–131, Neyman states, "I began as a quasi-Bayesian. My assumption was that the estimated parameter (just one!) is a particular value of a random variable having an unknown prior distribution."

4. Loc. cit.

HONORS AND DISTINCTIONS

HONORARY DEGREES

1959	D.Sc., University of Chicago
1963	LL.D., University of California, Berkeley
1964	Ph.D., University of Stockholm, Sweden
1974	D.Sc., University of Warsaw, Poland
1974	D.Sc., Indian Statistical Institute

MEMBERSHIPS

1963	U.S. National Academy of Sciences
1963	Royal Swedish Academy
1966	Polish National Academy
1979	Royal Society (London)

AWARDS

1958	AAAS Newcomb Cleveland Prize
1966	U.K. Royal Statistical Society Guy Medal in Gold
1969	U.S. National Medal of Science
1973	Copernicus Society of America, Medal and Award

SELECTED BIBLIOGRAPHY

I. Original Works

A complete bibliography of Neyman's work is given at the end of David Kendall's memoir (*Biographical Memoirs of Fellows of the Royal Society*, vol. 28, 1982).

Some of the early papers are reprinted in the two volumes, *A Selection of Early Statistical Papers of J. Neyman* and *Joint Statistical Papers of J. Neyman and E. S. Pearson* (University of California Press).

Neyman's letters to E. S. Pearson from 1926 to 1933 (but not Pearson's replies) are preserved in Pearson's estate.

An overall impression of Neyman's ideas and style can be gained from his book, *Lectures and Conferences on Mathematical Statistics and Probability*, Second Revised and Enlarged Edition (Graduate School, U.S. Department of Agriculture, Washington, D.C., 1952). The following partial list provides a more detailed view of his major theoretical contributions.

A. Paradigmatic Papers

1928

With E. S. Pearson. On the use and interpretation of certain test criteria for purposes of statistical inference. *Biometrika* 20A:175–240, 263–94.

1933

With E. S. Pearson. On the problem of the most efficient tests of statistical hypotheses. *Philos. Trans. R. Soc. Lond. Ser. A* 231:289–337.

1934

On the two different aspects of the representative method. *J. R. Stat. Soc.* 97:558–625 (A Spanish version of this paper appeared in *Estad. J. Inter-Am. Stat. Inst.* 17:587–651.)

1937

Outline of a theory of statistical estimation based on the classical theory of probability. *Philos. Trans. R. Soc. Lond. Ser. A* 236:333–80.

B. Some other theoretical contributions

1937

'Smooth' test for goodness of fit. *Skand. Aktuar. Tidskr.* 20:149–99.

1939

On a new class of 'contagious' distributions, applicable in entomology and bacteriology. *Ann. Math. Stat.* 10:35–57.

1948

With E. L. Scott. Consistent estimates based on partially consistent observations. *Econometrica* 16:1–32.

1949

Contribution to the theory of the chi–square test. In *Proceedings of the Berkeley Symposium on Mathematical Statistical Probability*, ed. J. Neyman, pp. 239–73. Berkeley: University of California Press.

Optimal asymptotic tests of composite statistical hypotheses. In *Probability and Statistics*, Harald Cramér Volume, ed. U. Granander, pp. 213–34. Uppsala, Sweden: Almquist & Wiksells.

Neyman's position regarding the role of statistics in science can be obtained from the following more philosophical and sometimes autobiographical articles.

1951

Foundation of the general theory of statistical estimation. *Actual. Sci. Ind.* 1146:83–95.

1955

The problem of inductive inference. *Commun. Pure Appl. Math.* VIII:13–45.

1957

'Inductive behavior' as a basic concept of philosophy of science. *Rev. Inst. Int. Stat.* 25:7–22.

1959

With E. L. Scott. Stochastic models of population dynamics. *Science N.Y.* 130:303–8.

1970

A glance at some of my personal experiences in the process of research. In *Scientists at Work*, ed. T. Dalenius, G. Karlsson and S. Malmquist, pp. 148–64. Sweden: Almquist & Wiksells.

1971

With E. L. Scott. Outlier proneness of phenomena and of related distributions. In *Optimizing Methods in Statistics*, ed. J. S. Rustagi, pp. 413–30. New York: Academic Press.

1977

Frequentist probability and frequentist statistics. *Synthèse* 36:97–131.

II. Secondary Literature

The most important source for Neyman's life and personality is Constance Reid's *Neyman—from Life* (Springer, New York, 1982), which is based on Neyman's own recollections (obtained during weekly meetings over a period of more than a year) and those of his colleagues and former students and on many original documents. A useful account of his collaboration with E. S. Pearson was written by Pearson for the Neyman Festschrift, *Research Papers in Statistics* (F. N. David, ed., Wiley, New York, 1966). Additional accounts of his life and work are provided by the following papers.

1974

L. Le Cam and E. L. Lehmann. J. Neyman—on the occasion of his 80th birthday. *Ann. Stat.* 2:vii–xiii.

1982

D. G. Kendall, M. S. Bartlett, and T. L. Page. Jerzy Neman, 1894–1981. In *Biographical Memoirs of Fellows of the Royal Society*, vol. 28, pp. 379–412.

1982

E. L. Lehmann and Constance Reid. In memoriam – Jerzy Neyman, 1894–1981. *Am. Stat.* 36:61–62.

1985

E. L. Scott. Neyman, Jerzy. *Encycl. Stat. Sci.* 6:214–23.